LETTERS TO THE EDITOR.

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The Origin of the Cultivated Cineraria.

IT appears to me that Mr. Bateson very imperfectly appreciates the nature of the problem of which he has hazarded what I venture to think an ill-considered solution.

In my last letter I pointed out briefly the grave objective difficulties which he had to face in substantiating his case. As Mr. Bateson is, by reputation, a serious naturalist, I think it was his duty to take up the challenge which I virtually threw down to him, and deal with the points which I brought under his consideration. This he has not chosen to do, but falls back again upon his "historical evidence" and his dialectic.

Now I must confess that I am myself as much bored as I suppose most people must be with the "modern Cineraria." And I grudge the time demanded for the discussion of a point which I brought forward as a merely incidental illustration. It may, however, be useful in saying all that I intend to say in reply to Mr. Bateson, to make a few general remarks on the whole subject.

It is apparently the fashion nowadays for the younger biologists to undertake the reconstruction of the Darwinian theory. The field is undoubtedly open, and posterity may safely be trusted to appreciate the value of their labours. But I cannot but observe that as between them and the author of the theory, there is this difference. Mr. Darwin, as he has told us, spent the best part of his life in studying patiently and sifting critically a vast mass of observation and fact. Ultimately he permitted himself to draw certain conclusions. The result is that if you take any statement which Mr. Darwin has put forward, you may feel assured that behind it is a formidable body of carefully considered evidence not likely to be upset. With the modern writers on evolution, the position is exactly the opposite. They launch their theories gaily on the world, and on demanding their substratum of facts, one is told that that is a matter for future collection. I myself am old-fashioned enough to think that, of the two methods, that of Mr. Darwin is the sounder, the more scientific, and in the long run the more convincing.

I have pointed out again and again the vast wealth of material for the scientific study of variation which is presented every day to the eyes of any one engaged in horticultural practice. The difficulty is that few persons possess either the scientific capacity, the patience, or the leisure for its profitable utilisation. We want, in fact, for the purpose a second Darwin, or at least a Herbert.

In his "Variation of Animals and Plants under Domestication," Mr. Darwin made a use which was remarkably effective of the observations made by "practical men" in horticultural literature. They served his purpose in establishing, as had never been done before, the amount and character of the variation which was possible under artificial conditions, and therefore, by analogy, under natural. But this class of evidence appears to me unsatisfactory for the investigation of the further problem which is at the moment of supreme interest, the nature and laws of variation itself. I think that Mr. Darwin squeezed out of it all that it would profitably yield. And for this reason: the evidence is not scientific—that is to say, it was never drawn up by persons having in view the requirements of scientific exactitude. Those who gave it have been pressed into court in a cause in which they never contemplated engaging. This has the merit of ensuring that their evidence is unbiassed, but it does not allow of its being pushed further than what it is capable of proving.

The defects of horticultural evidence may be illustrated in a variety of ways. One or two will suffice. In the first place, is the weakness of its nomenclature. Horticulturists are not, for the most part, skilled botanists. When they give a plant name, it is impossible to be sure that it is what a technical botanist would accept. It is as if one were reading the writings of a chemist, and when he mentioned potassium, the doubt occurred as to whether it was not lithium which was intended. I do not mean to imply any censure on the horticulturists; they use names current at the moment which are good enough for practical purposes, though they will not stand a critical test. But in after years no technical botanist would dream of accepting them as unimpeachable.

Again, it has often been found that where remarkable hybrids have been recorded, it has been ascertained later that no cross has in point of fact been effected at all. Yet the original announcement will be quoted, and often has been as an undoubted evidence of the fact.

I arrive, then, at the conviction that if any profitable use is to be made of horticultural experience in the study of variation, the so-called historical evidence will have to be discarded. Every step of the investigation must be made under the actual eye of a competent observer, and nothing taken at second-hand.

I will now return to the Cineraria. The feral form had been long lost to cultivation, but some years ago it was reintroduced to Kew from the Canaries. Mr. Rolfe, a member of my scientific staff, illustrated it in the *Gardeners' Chronicle* in 1888, and pointed out the striking changes which it had exhibited under cultivation. These have subsequently interested me because I have been endeavouring to collect facts as to the rate of variation.

Now Mr. Bateson, solely on what he calls historical evidence, still asserts, and in the face of the difficulties which I have pointed out that such a theory presents, that the modern Cineraria is of hybrid origin. Very well; let us assume that as a provisional hypothesis. How is it to be tested? It is easy to see from an analogous case. The horse and the zebra have been crossed; are we justified in asserting that the last winner of the Derby is of zebra descent? The criteria are two, and I think two only: (1) an uncontested pedigree; (2) palpable marks of parental characters.

Now, with regard to (1), practically in plants it cannot be obtained. We can only fall back upon "historical evidence." I have attempted to show above, in a general way, how little scientific value can ordinarily be attributed to this. One cannot be sure that the asserted parents were what they are stated to be. But my object was not to undermine the weight of what Mr. Bateson has brought forward. I accept it and reject it as wholly irrelevant. As my friend Prof. Rolleston was fond of saying, it would be valueless evidence even to convict a poacher.

The fact that certain shrubby Cinerarias with hoary leaves and one with yellow flowers were crossed (if they really were) early in the century, proves nothing as to the existing Cineraria, any more than the cross between the zebra and the horse does as to the parentage of any existing horse.

These shrubby Cinerarias were, as Mr. Bateson states, propagated by cuttings (they are not too easy to strike); and like many other interesting plants, they disappeared from all but botanic gardens towards the middle of the present century.

As I am quite unable, then, to attach any weight to the so-called historical evidence, because I fail to see that it establishes any filiation between the plants with which it deals, strikingly different as they are, and the plant with which I am dealing, there is nothing left but to try (2), and see what evidence of its parentage the plant itself affords.

Now, it is well known that organisms of hybrid origin preserve, in some degree, their parental characters, and this has even been shown to be true of their histological elements. Modern taxonomic botany has met with considerable success in the analysis of plants of hybrid origin into their constituents. The Floras have in consequence been cleared of a multitude of dubious plants, the real nature of which can now be accounted for. And the validity of the method has been established by the results of a corresponding synthesis. We had, then, no hesitation at Kew in applying the test to the Cineraria. Although it had often been examined before, with the assistance of some members of my staff I made a fresh examination. I took copious specimens of *Cineraria cruenta*, and of an average cultivated form, and carefully compared them point by point. Except in the multiplication of the florets in the heads, especially of the ray-florets, we could distinguish no tangible morphological difference. In fact, having accidentally mixed up leaves belonging to the two parcels, I found myself unable with any certainty to refer them back again. This is pretty conclusive evidence of the actual morphological identity of the vegetative organs of the two plants.

The next thing was to compare the cultivated Cineraria with its reputed shrubby "historical" parents. These present well marked and somewhat peculiar characteristics not readily described in non-technical language. But the cultivated Cineraria does not present the smallest trace of any one of them. As far, then, as the matter admits of investigation at all by any known methods, I regard the conclusion which is generally accepted here as a sound one. At any rate, it rests on a careful

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consideration of the objective facts which Mr. Bateson wholly shirks.

I now come to the other point. I put colour change entirely aside for reasons which seem valid to me, and which I may take another opportunity of explaining. Apart from these the cultivated Cineraria exhibits no variation from the feral form which may not be described as dimensional. While the foliage has remained approximately constant, the loose corymbose habit has been contracted into a tight corymb, and the heads of florets have been enormously enlarged. While the feral form stands about five feet high, the cultivated one is about eighteen inches. I am disposed to restrict the term "sporting" to a definite morphological change such as is exhibited in the flowers of the garden Chrysanthemum, and recently in the occurrence of an "ivy-leaved" form of the Chinese Primrose. But except a race of so-called double Cinerarias, which did not take the public fancy, the history of the garden Cineraria does not present, as far as I know, any trace of a real morphological change. If I might venture to use a mathematical analogy, I should say that the form of the Cineraria-function has remained unaltered.

Now the object of these dimensional changes has been to make the plant worked upon handy and convenient for decorative purposes. Those points which were unessential for this purpose have been unconsciously neglected, and their stability has not been affected. But I do not doubt that if it had been otherwise the Cineraria might have been brought by this time to any configuration which the cultivators fancied.

As far as I can make out, the transformation of the Cineraria has taken about sixty years to effect. Mr. Bateson will not complain if I quote a few words from one of his own authorities of about that date:—"One species especially merits cultivation, viz. *C. cruenta*. This may be regarded as the parent of many of those beautiful varieties which are so successfully cultivated by Messrs. Henderson." Now my memory of the cultivated Cineraria goes back some thirty years. I can remember when it was a rather lanky plant, about half the height of the feral form, with a somewhat lax inflorescence and far smaller flower-heads than are now to be seen. The present fashionable Cinerarias, with a very condensed inflorescence and very large flower-heads, only date back some ten or twelve years.

I see, therefore, no reason for abandoning my assertion that the evolution of the modern Cineraria has been slow and gradual, and not *per saltum*, and this is in accord with general horticultural experience. As soon as a new plant is introduced, every one wants to get a form with bigger flowers or floral structures than anybody else. There is only one secure path to this result, and that is by taking advantage of seminal variation and selecting the minutest trace of change in the desired direction. By patiently and continuously repeating the operation, almost any desired result can be obtained. The horticultural gambler may hope to reach it by a "sport," but he will not. *Anthurium scherzerianum* is a good illustration. Introduced in 1862, it was little more than a curiosity; now its enormous and brilliant spathes are a conspicuous object at every flower-show. This has simply been accomplished by progressive selection working on seminal variation.

Mr. Bateson has now the coolness to say that "the hybrid origin of cultivated Cinerarias is of subordinate interest." All I can say is that in that case it is a pity that he wasted three columns of NATURE with a discussion of the subject. I should have thought myself that it was a matter of very considerable importance indeed to be able to form an approximate idea of the amount of change in a given time in an unmixed species, and so obtain some measure of the possible rate of evolution, at least in regard to dimensional characters.

For my part, I think that in the study of evolution we have had enough and to spare of facile theorising. I infinitely prefer the sober method of Prof. Weldon, even if it should run counter to my own prepossessions, to the barren dialectic of Mr. Bateson. W. T. THISELTON-DYER.

Royal Gardens, Kew, May 13.

Some Bibliographical Discoveries in Terrestrial Magnetism.

I HAVE recently made some interesting discoveries pertaining to the history of Halley's famous chart of the Lines of Equal Magnetic Variation (Declination), to which renewed attention is just now being called by Prof. Hellmann's admirable facsimile reproduction of the earliest geomagnetic charts.[1]

The first reproduction in facsimile of Halley's chart was undertaken by G. B. Airy, and published in "Greenwich Observations" for 1869. Airy was led to do this by reason of the fact that he could find no geomagnetician of his time who had ever seen Halley's chart. After diligent inquiry among academies and libraries at home and abroad, it was found that the British Museum possessed a copy, and, it was believed, the *only* copy extant. Since then, Prof. Hellmann has succeeded in tracing two other copies, one at Hamburg (Stadt Bibliothek) and one at Paris (Bibliothèque Nationale), and has also, since the publication of his book (as he has just informed me), come into possession of a copy himself.

I have personally examined the Hamburg and Paris copies, and, during a brief stay in London in March, also the copy in the British Museum used by Airy. I have found, moreover, in the British Museum, three other Halley charts and two Dutch reprints. By a careful and critical study of these various copies, some new light is thrown upon the publication of Halley's chart. To make this apparent, some wearisome details with regard to the various copies will be necessary. I will begin with the British Museum copies.

Catalogue No. 974 (5).—"A new and correct Sea-chart of the Whole World, showing the Variations of the Compass as they were found in the Year 1700, by Edmund Halley." Date (according to the Catalogue), 1701.

The above is the English title of the chart referred to at times by the Latin title, "Tabula Nautica," &c. This copy appears to be the one used by Airy in his facsimile reproduction of the Halley chart published in "Greenwich Observations" for 1869, which in turn has been used for Prof. Hellmann's reproduction. There is no date on the chart, nor the name of the publishing firm. The date 1701, assigned hitherto, is probably due to Halley's defence of his chart, contained in *Phil. Trans.* vol. xxix. (Unabridged), 1714. Halley says, p. 165, "to examine the chart I published in the year 1701, for shewing at one View the Variations of the Magnetical Compass, in all those Seas with which the English Navigators are acquainted." But we find that the above number is dedicated "To his Royal Highness, Prince George of Denmark, Lord High Admiral of England, Generalissimo of all Her Majestie's Forces." As Prince George, consort of Queen Anne, did not bear this title until April 17, 1702,[2] it is evident that the above number is either not the original Halley chart published in 1701, or it is a reprint with a later dedication. If it is to be regarded as an original Halley chart (not a reprint), then a date between 1702 and 1708 must be given it, as Prince George died October 28, 1708. It was published probably not far from 1702, and is in excellent condition.

No. 973 (15). Same title as previous number. Date given in the Catalogue, 1720(?) I found upon examination that this is identical with No. 974 (5). The Catalogue date is doubtless erroneous. This copy is cut into sections and remounted.

No. S. 112 (6). This is a large folio atlas containing a reprint of No. 974 (5), bearing now the name of the publishing firm, R. Mount and T. Page, and having in addition an extra strip, from 90° to 160° E. of London, pasted on the left-hand side, so that the chart now embraces 430° of longitude instead of 360° as before. The Hamburg and Paris copies are exact duplicates of this, the only difference being that they have pasted below a strip bearing the explanation of the chart by Halley. Prof. Hellmann, in the work cited, has given us the Hamburg text. The Paris text differs in the orthography of a few words, and in the spacing of some of the lines. It appears to be the older text, as below it we find the name of the firm as R. and W. Mount and T. Page, while the name of the firm on the Hamburg text is Thomas Page and William Mount, and the former I have ascertained to have been the earlier firm. This English text I have failed to find attached to the British Museum copies.[3]

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[1] Neudrucke von Schriften und Karten Über Meteorologie und Erdmagnetismus. Herausgegeben von Prof. Dr. G. Hellmann, No. 4. . . . E. Halley, W. Whiston, J. C. Wilcke. A. von Humboldt, C. Hansteen: Die ältesten Karten der Isogonen, Isoklinen, Isodynamen; 1701, 1721, 1768, 1804, 1825, 1826. 4to. 26 pp. 7 plates. (Berlin: A. Ascher and Co., 1895.)

[2] Rapin de Thoyras's History of England, London, 1751, vol. iii. 1689-1707, p. 544.

[3] The atlas contains, besides "An Account of the Methods used to describe Lines on Dr. Halley's Chart of the Terraqueous Globe," &c., by W. Mountaine and J. Dodson, London, 1758, and copies of the Halley chart revised for epochs 1744 and 1756. It bears the title on the back: "Tabulæ Nauticul Variationes Magneticas Denotentes. E. Halley." It appears to be a compilation of charts, probably by the authors (Mountaine and Dodson) of the revision.

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